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alveolar rows finally break in the cleavage of the cytoplasm. Meanwhile the vacuole-like nucleus has been pulled apart towards the centrosomes.

After the cleavage of the cell the centrosome again ceases to absorb liquid and so passes into the resting stage. The case where there is an immediate division of the centrosome into two that remain for the subsequent cell divisions also admits of explanation upon this alveolar basis; the same is true of various other cases and phenomena.

If this attempt at an understanding of the complex marvels of cell division appears much too ineffectual it is partly due to the imperfect representation given in this abstract and in part the result of the unfinished character of the present paper which claims to be but the first of a series. In the next article the author hopes to consider the nucleus with its chromosomes and spindle; and we cannot well judge of the success or failure of the attempt till that part lies before us.

Probably few will judge that much ultimate truth has yet been discovered in the attempted explanation of such exceedingly complex phenomena but if it prove that the right line of research has been struck the author has added much to our conceptions of the forces at work in embryological processes.

## PSYCHOLOGY.[1]

**Experiment on Reinversion of the Retinal Image.**—The inversion of the image on the retina, and its influence upon our visual perception of space, have given rise to considerable discussion in the past. That we see things in an upright position notwithstanding this inversion, has seemed to many writers to require special explanation. Accordingly, some have assumed a reinversion of the image in the cerebral cortex, while others have adopted a theory of visual projection which makes the retinal inversion essential to upright vision. During all this discussion the possible relativity of *up* and *down* escaped notice for a long time. What we mean by down is simply the *ground side*, and by up simply the *sky side*. As everything imaged on the retina is inverted, there is no point of reference to give an indication of the inversion of the rest. The only problem that arises, then, is concerning the co-ordination of visual with tactile space. This is a real difficulty: I see my left hand *down* at my *left* side; I feel it in the same place. How can this co-ordination be reconciled with the fact of retinal inversion? There have been attempts to explain the situation in two different ways; either on one of the theories mentioned above, which assume the subjective space-scheme to be something absolute and rigid, and which postulate a cortical or projective re-inversion of the visual figure to conform with the uninverted tactile figure; or else by supposing the space-scheme plastic, so that it is capable of being determined, or at least "oriented," by experience. According to the latter view the visual and tactile "spaces" are not necessarily the same in origin; they have come to coincide only through habitual association.

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[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

Dr. G. M. Stratton, of the University of California, reported at the recent Psychological Congress at Munich an experiment of his own, which was, so far as it went, a crucial test between these two lines of theory. The details have since been published in the *Psychological Review*, (Vol. III, pp. 611–617), from which I quote. By means of a pair of convex lenses, he succeeded in inverting the field of vision without otherwise altering its relations. The effect of this contrivance, when placed to the eye, was to give a re-inverted (or upright) retinal image. For the experiment, the apparatus was bound to the face in such a way as to exclude from the right eye all light except that passing through both lenses, and the visual field of the other eye was darkened. The observer wore the apparatus constantly for two days, except at night, when his eyes were carefully bandaged; so that during all this time he saw only the inverted field of vision.

"The course of experience," says the author, in reporting the results, "was something as follows: All images at first appeared to be inverted; the room and all in it seemed upside down. The hands when stretched out from below into the visual field seemed to enter from above. Yet although all these images were clear and definite, they did not at first seem to be real things, like the things we see in normal vision, but they seemed to be misplaced, false, or illusory *images* between the observer and the objects or things themselves. For the memory-images brought over from normal vision still continued to be the standard and criterion of reality. The present perceptions were for some time translated involuntarily into the language of normal vision; the present visual perceptions were used simply as signs to determine how and where the object would appear if it could be seen with restored normal vision. Things were thus *seen* in one way and *thought of* in a far different way. This held true also of my body. * * *

"As I moved about in the room, the movement of the visual images of my hands or feet were at first not used, as in normal vision, to decide what tactual sensations were to be expected. Knocks against things in plain sight were more or less of a surprise. I felt my hand to be in a different position from that in which I saw it, and could not, except by cool deliberation, use its visual image as a sign of impending tactual experience. After a time, however, repeated experience made this use of the visual image much less strange; it began to be the common guide and means of anticipation. I watched my feet in walking, and saw what they were approaching, and expected visual and tactual contact to be reported perceptionally together. In this way the limbs began actually to feel in the place where the new visual perception reported them to be. The vivid connection of tactual and visual perceptions began to take away the overpowering force of the localization lasting over from normal vision. The seen images thus became *real things* just as in normal sight. I could at length *feel* my feet strike against the *seen* floor, although the floor was seen on the opposite side of the field of vision from that to which at the beginning of the experiment I had referred these tactual sensations. I could likewise at times feel that my arms lay between my head and this new position of the feet; shoulders and head, however, which under the circumstances could never be directly seen, kept the old localization they had had in normal vision, in spite of the logical difficulty that the shape of the body and the localization of hands and feet just mentioned made such a localization of the shoulders absurd.

"Objects lying at the moment outside the visual field (things at the side of the observer, for example) were at first mentally represented as they would have appeared in normal vision. * * * But later I found myself bringing the representation of unseen objects into harmonious relation with the present perception. They began now to be represented nos as they would appear if normal vision were restored, but as they would appear if the present field of vision were widened or moved so as to include them. * * *

"As to the relation of the visual field to the observer, the feeling that the field was upside down remained in general throughout the experiment. At times, however, there were peculiar variations in this feeling according to the mental attitude of the observer toward the present scene. If the attention was directed mainly inward, and things were viewed only in indirect attention, they seemed clearly to be inverted. But when, on the other hand, full attention was given to the outer objects, these frequently seemed to be in normal position, and whatever there was of abnormality seemed to lie in myself, as if head and shoulders were inverted and I were viewing objects from that position, as boys sometimes do from between their legs. At other times the inversion seemed confined to the face or eyes alone.

"On removing the glasses on the third day, there was no peculiar experience. Normal vision was restored instantaneously and without any disturbance in the natural appearance or position of objects."

As the author remarks, the experiment did not cover enough time to determine the full power of experience. But the main point at issue—the two opposing views of subjective space already alluded to—seems to have been fairly well settled. There was shown to be a possibility of co-ordinating our tactile space with an artificially inverted visual space; the localization of the hand (*e. g.*) by *feeling* was gradually assimilated to its position in the new visual field, etc. There was not, of course, time to overcome completely the experience of ages in one direction. But that it was overcome at all, and that the new experience was to a large degree reconcilable with the tactile data, shows clearly, I think, that the original co-ordination is quite independent of any mental projection or cerebral re-inversion. The two space-schemes arise separatively; that they correspond, point for point, as they do, is owing simply to repeated and uncontradicted experience.

It is to be hoped that the author will be able to repeat the experiment again and observe the effect of a longer continuation of the experience. He might be able eventually to get rid of the persistent inversion of the head and shoulders, perhaps, by looking at himself frequently in a mirror. If a single pair of lenses could be devised to cover both eyes, the difficulties arising from imperfect convergence might be overcome, and the experiment extended to binocular vision.

HOWARD C. WARREN.

**Birds' nests and instinct.**—Some very interesting observations on the nesting habits of birds recorded by Dr. R. Williams in the October (1896) number of the Zoologist are worthy of note, the more so since they bear upon the subject of instinct and the power of learning from experience that has lately been occupying the attention of certain psychologists. It must be regretted that Dr. Williams' observations seem to be hap-hazard and, therefore, lacking the detail that would make them conclusive. In this respect he is not different from the host of oological observers that claim the title of scientists. Take for instance his note regarding cuckoos hunting their nests. He says "my son informed me one day that he knew where a cuckoo was going to lay. He said he had seen a cuckoo fly out of a hedge, which on examination he found to contain a hedge-sparrow's nest ready for eggs. The boy's expectation was realized. He visited the nest repeatedly, and one day announced that the Hedge-sparrow had begun to lay. Three days later he brought me the contents of the nest, consisting of one cockoo's egg and the clutch of hedge-sparrow's eggs." Now, it may be asked, how, from this account, is one to be sure that the egg was laid by the cuckoo that was seen to fly out of the hedge? There is no statement that the bird was watched from day to day, or that it was known to roost and feed in that immediate vicinity. No one is said to have gone early in the morning to the roosting tree and from that time follow and note the actions of the bird through the day until it went to roost again for the night. It is improbable that such observations were made in this case in this way. Yet it is not impossible to make them thus carefully, for it has been done in studying the feeding habits of our American cuckoo.

Aside from this criticism the observations are very interesting so far as they go. For instance in 1889, he found the nest of a European blackbird situated in a depression in the ground very much as one usually finds the nests of the skylark. Several other blackbirds nests were found by the keeper of the wood, which were similarly placed. Two thrushes nests were also found in the neighborhood, and like those of the blackbirds on the ground. The wood abounded in thickets and fir trees, but these more favorable places contained very few blackbirds or thrushes' nests. It was learned upon inquiry that the proprietor finding the wood a stronghold for these species had made systematic raids upon their nests in consequence of their destruction of his fruit. Did the birds profit by experience and seek a safer position for their nests?

The other instance to be noted concerns the sandpiper. "In May, 1886, just when these birds were commencing to set," the author says, we had a very heavy rainfall, heavier than any remembered by my father, who is over eighty years of age. The land on each side of the river near my house was under water. The common sandpiper usually nests on patches of gravel thrown up by the water, and more or less covered with docks and other weeds. These places being flooded, the nests were swept away and destroyed. On the subsidence of the flood, the sandpipers built again on their old sites, only to find their nests swept away by another flood. In the nesting season of the following year (1887), wishing to secure a few clutches of sandpiper's eggs, I searched in the usual places for a whole morning without success. The next day I accidently came upon a sandpiper's nest containing four eggs, the nest being placed at the foot of a willow fully 100 yards from the river. The discovery put me on the right track, and I found six more nests in various positions, all a long way from the river." Evidently this was profiting by experience and the conclusion is borne out by the fact that the birds seemed to conclude that the new position was not so favorable as the old to which next year they returned.

His numerous other observations are interesting but space permits noting only the case of a wood warbler—which usually makes a covered nest—making a nest in a situation such that the top was formed by the root of a tree.—F. C. KENYON.

**Psychic Evolution.**—In the paper by Mr. Nichols which is concluded in this number of the NATURALIST, is to be found the Neodarwinian doctrine of psychic evolution, which is also adopted by Prof. W. H. James. I have already criticised this doctrine as expressed by Prof. Mark Baldwin in a paper in the NATURALIST (1895, April, p. 342; May, pp. 422–28). This doctrine is in short, that structures come into existence before the functions which they represent, and it is, therefore in direct opposition to the Lamarckian view, that structures are the result of functions. Of course the Lamarckian view does not deny that completed structures perform their functions better than they can be performed by unspecialized structures. This doctrine has the distinct advantage as a hypothesis, of really doing what a doctrine of evolution is supposed to do, i. e. of explaining the origin of structures. This the Neodarwinian doctrine does not do. As a theory of origin of organs of specific consciousness and of their functions, it is quite the same whether we assume with Prof. James that central organs must first exist, or whether we believe with Prof. Wundt that special end-organs must first exist. The fact is that a doctrine that assumes that any organs must be primary and not secondary is in so far not a doctrine of evolution.

The evolutionary doctrine of the origin of organs of special consciousness must be the same as that which explains the origin of other organs. That is, that energies acting as stimuli, external and internal, impinging on live protoplasm, produce modifications of its structure. If these structures are concerned directly or indirectly in the production or modification of states of consciousness, their use, or the subjection of the tissues to the stimuli which produce them, will produce pleasure or pain. The organism proceeds to repeat or avoid the exposure thereafter accordingly, and use and disuse have their beginning.

Whether any form of general and diffuse consciousness preceded in time special forms of consciousness, or whether all forms of consciousness have been due to corresponding stimuli, is a subject of present research. It can be assumed with much probability however, that the stimuli of pressure, heat, light, and hunger, would produce different forms of consciousness, since they would produce different effects on the ultimate structure.

That this is the proper order of psychic evolution may be inferred from two considerations. (1) The sensations due to pressure, light, heat, hunger, etc. are experienced by animals of low as well as of high organization. (2) The order of structural evolution is from the homogeneous to the heterogeneous, or from the generalized to the specialized.—E. D. COPE.

## MICROSCOPY.

**Formal,[1] (Formaldehyde 40 per cent. solution).**—Practical experience with Formal in the Laboratory. By Bert B. Stroud, D. Sc., Instructor in Physiology, Vertebrate Zoology, and Neurology. Cornell University, Ithaca, N. Y.

During the past two years much interets has been shown concerning formaldehyde, as an agent for hardening and preserving anatomic material. An extensive literature has arisen, which as Lee[2] has observed, is sadly confused by the indiscriminate and incorrect use of the terms *Formol* and *Formalose.* The term *formol* is a very bad one. Because according to the principles of chemical terminology the suffix *ol* is applied to a different class of compounds, examples of which are alcohol, glycerol, and phenol.

The molecular structure of the aldehydes differs from that of the examples given.

*Formalose* might be confused with *formose,* a mixture of carbohydrates made from formaldehyde. *Formalin* is a term which has no meaning.

We have to deal with a definite chemical individual; why then should it not be called by its own proper name or by a suitable contraction of that name? It may be urged since the commercial product is a solution of gaseous formaldehyde in water, that the name should indicate such solution. But the term *hydrochloric acid* is an excellent precedent for saying formal or formaldehyde, since the acid of the laboratory is a solution of the gas HCl in water. But every one calls the solution hydrochloric acid.

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[1] The term *Formaldehyde* is a cumbersome one. Professor W. R. Orndorff has suggested that *Formal,* from *Formal-dehyde* is the best scientific contraction. The term *Methanal,* for formaldehyde, was adopted by the Geneva Congress of Chemists.

[2] Formol or Formaldehyde? *Anatomischer Anzeiger. XI, No. 8, October 24, 1895, S. 255–256.*